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What Do We Know About Economic Growth? Or, Why Don't We Know Very Much?

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Summary. — The last 10 years has seen an explosion in cross country econometric studies of growth, driven by two factors—new mathematical models of the growth process that lend themselves to econometric testing, and new data sets that make such testing possible. This paper looks at a selective review of these studies. It concludes that the results are disappointing in that no model has proven robust to trial by repeated regression. The paper suggests some reasons for this—including that the tested models tend to be ahistorical and over-simple in terms of their causal accounts. It concludes with possible lessons for econometric work in this area. © 2000 Elsevier Science Ltd. All rights reserved.

Key words — economic growth, theory, cross country regressions

1. INTRODUCTION [1]

In June 1996, *The Economist* magazine published a piece based on results from a global statistical study concluding that had African countries followed better policies, such as those followed in eight fast-growing economies over the last few decades, the region would have grown 4.6% per annum faster than its historical growth rate—indeed, faster than the comparator set of fast-growing economies. A year later, *The Economist* published another piece based on results from a global statistical study that concluded "for much of the world, bad climates, poor soils and physical isolation are likely to hinder growth whatever happens to policy." This study concluded that, even had Africa followed better policies, it would have grown 2.3% slower per year than the countries of South and South East Asia.

These two articles, with their markedly different conclusions, provide an illustration of the problems facing even the best development economists—indeed, both articles were written by Jeffrey Sachs (Sachs, 1996, 1997a). [2] Overall, attempts to divine the cause or causes of long-term economic growth, testing a wide range of possible determinants using statistical techniques, have produced results that (like the two Sachs articles) are frequently contradictory to results reported elsewhere. That is, empirical evidence is hardly unanimous in support of a particular view of the growth process.

If this is right, it might pose a serious problem for development practitioners. As others have argued, many of those concerned with stimulating growth in developing countries have a more or less universally applicable set of policy prescriptions for achieving that goal. The empirical evidence, however, seems to provide little firm guidance for the universal efficacy of any particular policy prescriptions. Indeed, we will argue that if the evidence shows anything at all, it is that markedly different policies, and markedly different policy mixes, may be appropriate for different countries at different times.

* The opinions expressed here are the authors' own and do not necessarily reflect those of the World Bank, its executive directors, or the countries that they represent.
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In addition to suggesting that at present we do not know all that much with any certainty about the causes of long-term economic growth, the paper tries to investigate the reasons for this. Our approach here is a focus on the structure of the theorizing about economic growth upon which these econometric studies have been based. In other words, we do not attempt to distill out of these studies the "real" cause or causes of economic growth; rather, we ask if there are features of the theorizing behind them which would explain why these studies have not produced universally compelling accounts of the process of economic growth. The argument of this paper is that there are good theoretical reasons to match the empirical evidence for thinking that the growth process is highly heterogeneous.

We argue that there is a crucial and important mismatch between the actual economic world, and the "picture" or "model" of the economic world which underpins the current search for causally significant variables. Specifically, we suggest that current thinking about economic growth has often failed to grasp the complex causal nature of the social world, assuming that the components and processes of the economy are the same across countries. Following from this, we think that any account which takes as an assumption that the process of economic growth works more or less unaltered across enough countries to be proved or disproved through the statistical testing of variables in large cross country regressions is likely to be inadequate.

Section 2 seeks to sketch out what we take to be some of the key features of theorizing about economic growth. It argues that the commitment to what we call here epistemological universalism entails a commitment to what we call ontological univeralism, and that these commitments underpin the current search for causally significant variables in the study of economic growth. Section 3 is a review of the available statistical evidence concerning the causes of economic growth. Section 4 of the paper ties the discussion together by suggesting some reasons why key features of the theorizing do not produce good empirical accounts of the process of economic growth. In Section 5 we suggest a number of possible implications for the current practice of development, and for the way we theorize about economic growth.

2. THEORETICAL AND PRESCRIPTIVE UNIVERSALISM

The discussion in this section hinges around the suggestion that there is a connection between the kind of knowledge we can hope to gain of a set of phenomena and the nature of that set of phenomena. For example, we are usually fairly confident that we can get law-like knowledge of processes in the natural world, whereas we are less sure we can do so of the social or political worlds. This is at least in part the result of the two kinds of world being different; we are fairly sure there are some universal causal mechanisms in nature (gravity, for example), but are less sure there are in, say, politics or in family life, or at least if there are such laws, it is not clear we are very near discovering them. Moreover, of course, part of the reason for this is that political and family life are inhabited and constructed by active, creative, and thinking persons, and it is not at all clear we have any universal causal laws covering their activities.

As regards current thinking about economic growth, this connection between the kind of knowledge we can hope for, and the nature of the phenomena under study, implies that the kind of knowledge we can hope to have of the growth process is determined by what we think economies and economic process are like. We suggest that, among many economists who study growth, there is a necessary commitment inherent in cross country econometric studies to two kinds of universalism as regards their view of the growth process.

First, there is a commitment to what might be called "epistemological universalism." This is inherited from the scientific revolution and involves a commitment to the production of "true" knowledge generated following the "rules" of science, which in turn provides the basis for prediction. This kind of knowledge is "universal" in the sense of not being a reflection of a particular time or place; it is, to use Nagel's phrase, a "view from nowhere" (Saiedi, 1993, p. 22; Nagel, 1986). As regards the study of economic growth it means a commitment to the idea that all economic processes everywhere are, in principle, knowable. That is, the aim of economics has been to provide theories generated following scientific rules, which explain the largest possible class of phenomena. This has been evident since at least Adam Smith, who was searching for the causes and mechanisms which would "connect together the otherwise

disjointed and discordant phenomena of nature" (Smith, 1980, p. 119).

This commitment to epistemological universalism entails a prior commitment to what might be termed "ontological universalism." This has two closely related elements. First, the "components" of all economies are in some way the same, and hence that economies and economic processes are comparable. Second, these components of the economy interact with one another in the same kinds of ways, thus producing certain economic "laws" or regularities which operate across all economies regardless of time or space. These two related commitments underpin the search for the causes of economic growth across countries, and the cross country statistical techniques used to discover them. If the components of economies were not in some way the same, or if they did not interact with one another in the same kinds of ways across countries, then cross-country comparisons would not be valid; they would not be a comparison of like with like. It is this commitment to "sameness," combined with epistemological universalism, which underpins the value placed on theoretical parsimony seen in discussions of economic growth. Only if all economies are fundamentally the same, in their components and their processes, does the search for fewer and fewer universal explanatory principles or "laws" make sense.

There is a good case to be made that these commitments have underpinned the search for the causes of economic growth since the beginning of economics, and have subsequently informed prescriptions about how to achieve it. We have already suggested they were present in Adam Smith. A chronological review of some recent models suggests that this belief has, if anything, become even stronger.

The Harrod–Domar model posited a concrete and linear relationship between investment and growth rates worldwide. It was used by many development economists (in a way unintended by Domar) to forecast GDP growth rates in the developing world, and estimate the level of foreign aid required to reach some target rate of growth. The World Bank's RMSM (Revised Minimum Standard Model), developed in 1972, was based on Harrod–Domar. This model argued that countries with a capital output ratio of, say, five, needed 5% of GDP going to investment for each 1% increase in growth. Thus, if investment reached 15% in that country, growth would climb to 3%. For a long time a country's capital output ratio was used by the Bank to calculate the foreign aid "requirement" of a country in order to reach a growth target. [3] Although capital output ratios were allowed to vary between countries, the simple mathematical model linking changes in the rates of investment with changes in growth rates were supposed to apply to all economies.

While a range of other theoretical treatments of the investment-growth relationship are somewhat more complex, investment was central to most early growth theories. Rostow's stages of growth, Nurkse (1953) balanced growth, and Lewis' unlimited labor model all featured a central role for capital accumulation (Meier, 1999).

Domar himself argued that his model was not appropriate for determining long-term growth rates, and supported instead the Solow-Swan neoclassical growth model which predicted growth rates were dependent, not on investment, but the rate of technological change (Easterly, 1998). Crucially, this model assumed that technology change occurred at the same pace worldwide. It therefore had little explanatory power when looking at the huge divergence in income growth rates across countries, and so the neoclassical model was largely abandoned in favor of what became known as "endogenous growth theory," in which the rate of technological change varied across countries depending on other factors (Ruttan, 1998).

For example, an early endogenous growth model of Paul Romer's argued that output was related to physical capital, labor, and knowledge, where the quantity of knowledge was connected with investment rates (Romer, 1993). In other words, investment in physical capital returned as the central proximate determinant of growth rates. Other models emphasized investment in learning. Lucas' (1988) formal model argued that human capital was *the* significant factor determining growth rates worldwide, while Rebelo (1990) suggested that, if countries invested in labor-augmenting human capital, returns to physical capital would not diminish, allowing growth to continue forever. This picked up on earlier cross country work that had emphasized the importance of education. In 1981, Easterlin had looked at growth experiences worldwide and concluded that it was the late extension of universal education in many countries that answered the question "Why isn't the Whole World Developed?" (Easterlin, 1981).

But Romer and Lucas' formalizations have also spurred an ever increasing number of long-term growth models that have moved beyond physical and human capital. For example, Grossman and Helpman (1991) produced a model that explored the role that trade might play in growth. These models produced a range of different conclusions, but many posited that policies could have an important impact on growth rates even over the long term. In tandem with these developments in high theory, the 1980s saw the evolution of what came to be known as the "Washington Consensus" on development policy. Among the policies linked with the consensus were devaluation, reduction of budget deficits and inflation, liberalization of prices and interest rates, and privatization. What connected these policies was a belief that governments had played an overactive role in promoting development, taking on tasks best left to the private sector, and abusing powers best left unused. Widespread government intervention was not only unnecessary to promote growth, it was the chief barrier to achieving that growth. In addition, many policy prescriptions for developing countries revolved around reducing government spending while curbing inflation (Polak, 1997; Walde & Wood, 1999).

In a more explicit way than earlier theories, the "Washington Consensus" was underpinned by a belief that all economies are similar and thus will respond in a similar manner to the same policy change. Some of those working in the field went as far as to deny the need for a separate discipline of "development economics" arguing that "once it is recognized that individuals respond to incentives, and that 'market failure' is the result of inappropriate incentives rather than of non-responsiveness, the separateness of development economics as a field largely disappears" (Kreuger, 1986).

That belief remains strong among many economists. But the difficulty of pushing through reforms based on the Washington Consensus, and the limited impact these had even in those countries who succeeded in implementing them, encouraged the profession to look for explanations in the realm of political economy. In particular, Douglas North's New Institutional Economics argued that underpinning economically successful regimes was a strong network of property rights, market structures and decentralized, democratic decision-making processes (North, 1992). Some more recent offshoots of this theory have suggested a large role for economic and social networks that promote trust, thus suggesting the importance of things such as equitable distributions of income and efforts to overcome ethnic divisions (Putnam, Leonardi, & Nanetti, 1993; Fay, 1993; Svennson, 1994; Grenato, Inglehart, & Leblang, 1996; Seabright, 1997). Although not all of these positions have been formally modeled to the same extent as earlier theories, their conclusions are clear enough to suggest, again, that these theories are seen to have universal policy applicability.

A simplified chronology of "what the mainstream considers is needed for development worldwide" through to the early 1990s might run as follows, then: physical capital, human capital, policy reform, institutional reform and social development. [4] It should be noted that, while these factors have been added to more or less formal models in approximately this order, they are *not* new to nonformal discussions of development—a point we shall return to. While the steady addition of different factors to the list presented in formal models does suggest, in one way, an ever-greater appreciation of the complexity of the growth process, they tend to have been added to basic models (especially in their more formal incarnations) in a sequential rather than parallel manner, and with the continuing assumption that the model is universally applicable.

Thus the current search for the cause or causes of economic growth appears to be frequently informed by a commitment to producing objective, scientific, and universal knowledge of economic growth, and this is underpinned by the view that all economies are substantially similar in their components and processes—that there is but one basic production function driving all economies at all times and in every time-frame.

It should also be noted that universal models of economic growth developed over the past 40 years are, in their particularities and recommendations, in frequent contradiction with one another. For example, models have proposed both limited intervention as opposed to government support for education and/or R&D (Rebelo, 1990; Romer, 1990), and openness as well as trade controls (Romer, 1990; Young, 1991). This alone might give pause to those who give advice based on model results, but it would be a remediable problem if empirical evidence clearly supported one model over the other.

3. THE EVIDENCE

This section reviews how models of the growth process underpinned by universalist commitments have held up to the empirical evidence, following the widespread belief that the acceptability of an economic theory is to be judged by its ability to explain and predict phenomena (Arrow, 1974; Friedman, 1953). [5] First, we will look at the predictive power of various economic models when it comes to forecasting future growth. Then we turn to the record of cross country econometric studies trying to explain past growth using regression analysis. Finally, we look at a number of events and country experiences that are hard to explain using some of the usual models and explanations of growth processes.

In econometric exercises, a comparison is made between the growth performance of a number of countries, and the country policies and characteristics that are considered significant determinants of growth. In effect, these studies test if, across a range of countries, and given certain other features of the economy, certain policies and characteristics are consistently associated with higher or lower growth rates. A review of these cross country regressions is therefore useful for testing views of what causes growth embodied in phrases such as "the Washington Consensus" (Williamson, 1996), or in formal models that tend to assume that period, country and sector differences are largely irrelevant. If multiple cross country studies over different time periods using different samples and different regression techniques nearly all suggest that a variable considered significant in a growth model is consistently and strongly related to growth, this provides support for the universal applicability of that model. Sadly, the answer that appears to emerge from such testing seems to be that growth is a more complex process than can be captured by universal models.

Looking first at economists ability to predict growth, Sherden (1998), among others, has collected fairly damning evidence on the reliability of short-term forecasts of economic variables, including economic growth rates. [6] His survey of recent studies suggests that just using last year's growth as an estimate (the "naïve forecast") produces results as good as the average forecaster. He also finds that no forecasters are consistently above average in their predictive powers, that Keynesians and non-Keynesians are equally good—or bad—and that the accuracy of economic predictions has not improved over time.

It might be argued that predicting long-term growth should be easier than divining volatile one-year growth outcomes. But the predictive abilities of early development economists regarding longer-term growth potential also appear to have been weak. Easterly and Levine (1997a) note that in the 1960s Africa was predicted to grow faster than East Asia by many if not most development economists. At the same time, the Soviet Union was being touted as a model for rapid development (Krugman, 1994).

This would not, in itself, be a cause for concern for recent theorists if early development economics had few common elements with contemporary development theory, but that is not true (Srinivasan, 1993). While the emphasis might have changed, almost all the major variable types that have at one time or another been thought of as a major determinant of growth are present as chapters in Arthur Lewis' *The Theory of Economic Growth*, first published in 1955 (Lewis, 1965): the right to reward, trade and specialization, economic freedom, institutional change, the growth of knowledge, the application of new ideas, savings, investment, population and output, the public sector and power, and politics. [7] It does not appear that we have discovered a missing "silver bullet" since then, at most it might be said that we have re-arranged the bullets in the magazine.

It is not be surprising then, that tests of modern development theories based on past performance of countries also suggest a fairly poor track record for theory. We will start with models that focused on investment which, as we have noted, is seen by many development economists (although not all: Morgan, 1967; Easterly, 1998) as a central determinant of growth (Lewis, 1965, 1984).

The RMSM-type model that posits a concrete, linear investment-growth relationship performs particularly badly. One recent estimate suggests that out of 138 countries, only five had a statistically significant relationship between one year's investment and the subsequent year's growth with a reasonable capital-output ratio of between two and five. Looking at a particular country, if the Harrod–Domar model had been correct, Zambia (with a reasonably high investment rate and large inflows of aid) would have a GDP per capita

income of perhaps $20,000 by 1994, instead of the actual level of around $600 (Easterly, 1998).

Even in studies that consider investment's impact on growth in the context of many other factors, the evidence is far from reassuring for Harrod–Domar, as we will see from our review of the econometric evidence. At the same time, we will try to elaborate the reasons behind poor results from regression analysis, and how the techniques' foibles are useful for determining the cause of weak support for model-based predictions.

Figure 1 displays the simple relationship between decade average investment as a percentage of GDP and decade average GDP growth across a number of countries over 1950–90. Each point represents decade data for one country—so that countries with 40 years of investment and growth data will produce four data points. [8] As can be seen, there is clearly more to explaining growth rates than investment rates.

A simple regression analysis allows us to learn that looking at the variation in investment across countries (without taking into account any other influences) can only account for about 5% of the variation in growth across countries—although the relationship is strong enough to suggest that we have found *something* here, in that the variables are significantly correlated. [9]

At the risk of repeating a basic course of econometrics, *what* we have found depends on a number of assumptions, however. First, we have assumed that it is investment that causes growth, not some other factor that is correlated with investment—or perhaps a factor that explains both the rate of investment *and* growth rates. One example of such a factor is the starting income per capita of a country in the sample. As we can see from Figure 2, there is a pretty strong relationship between income per capita at the start of a decade (measured on a log scale) and investment over the course of that decade—richer countries invest more. [10]

At the same time, there has been a divergence in incomes between rich and poor over the long term (a phenomenon we will explore in greater detail later). Perhaps poorer countries grow less for some reason unconnected with investment rates, and investment is only correlated with growth because it correlates with wealth. We need to test for the relationship between investment and growth *allowing* for initial income, presented in Figure 3. The relationship between income-adjusted investment and growth is still there. This suggests that investment is not only related to growth because it is related to income per capita.

Again, at the risk of repeating the obvious, conditioning variables can have a significant impact on the apparent relationship between factors in growth regressions. For example, we know that, on average, richer countries are growing faster than poorer countries, so that if we estimated the equation

$$\text{Growth} = C + \beta \times \text{initial income}$$

β would take a positive value. But when we allow for investment rates, and estimate the equation

$$\text{Growth} = C + \beta \times \text{initial income} + \chi \times \text{investment}$$

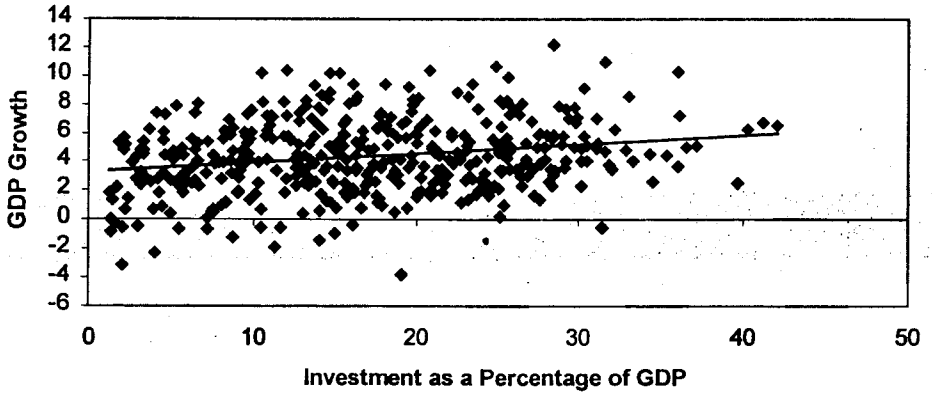

Figure 1. *The relationship between investment and growth.*

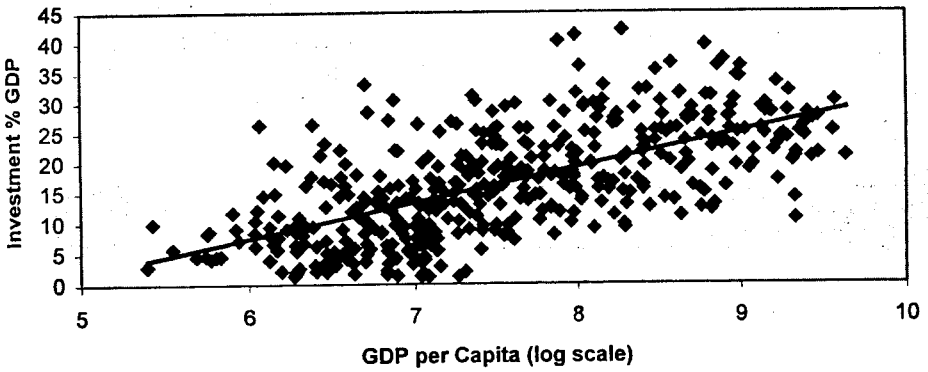

Figure 2. *The relationship between income and investment.*

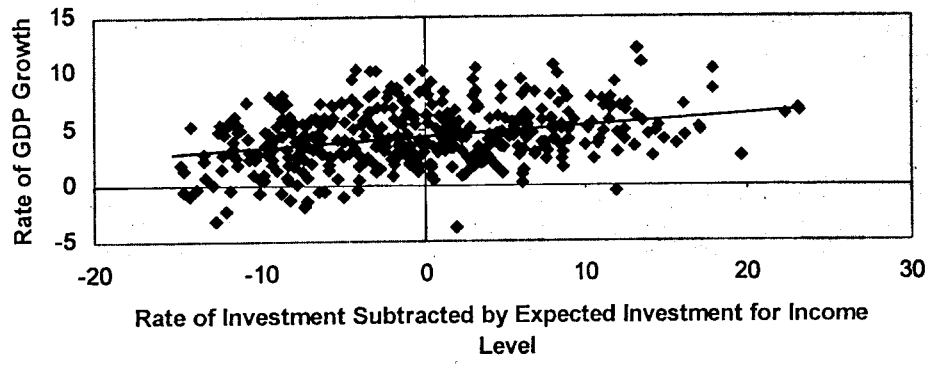

Figure 3. *The relationship between investment and growth corrected for initial income.*

β has a negative value while χ has a positive value (and one significantly larger than in a regression of growth versus investment alone). [11] This is the simplest form of the conditional convergence result—that country growth rates are inversely proportional to incomes (as would be predicted by the neoclassical model) if various other conditions are met.

Although not apparently a problem with the relationship between income, investment and growth, the above example does point up some potential problems with multiple regression analysis. Explanatory variables are frequently correlated themselves. For example, investment, education and a number of variables used to measure the quality of government institutions in a country are all positively correlated with income per capita. There are a number of possible explanations for such correlation. There is a chance that the apparent relationship is just luck—in fact, there is no causal relationship one way or the other between the variables. Given the number of variables that have been thrown into cross country regressions, we would expect that some would be correlated for just such a reason. But it might be that one variable has a causal relationship with the other (it is plausible to assume, for example, that people in richer countries can afford more education). It might be that both variables, while not directly related, are caused by something else (higher education and investment might be correlated purely because both are—in part—a product of higher income).

It is in the nature of regression analysis that, if two variables that are correlated with each other and with growth are entered into a growth regression at the same time, the apparent strength of their individual relationship with growth might appear much weaker than if one or other is entered alone. Levine and Renelt (1992) performed an exhaustive test

to discover how widespread a phenomenon this is. They took a number of variables commonly used in econometric analyses of growth and ran them in thousands of growth regressions with different conditioning sets of other variables, to see if the test variable remained significantly related to growth in all of them—which they defined as robust. Of the variables they tested, only investment survived as a robust variable. [12] This might suggest that all variables but investment have, at best, a relationship with growth dependent on a range of other conditions—or, at worst, they are only correlated with growth while the causal variable lies elsewhere.

In some ways, this might be an excessively restrictive test, however. While the test will fairly exclude variables that are only correlated with growth because they are caused by (or correlated with) another factor that itself has a causal relationship with growth, it will also exclude some factors that indirectly *do* have an impact on growth. For example, institutional variables are correlated with investment. Imagine that stronger government institutions encourage higher investment in a country (because there is a lower risk of high inflation or expropriation, perhaps), and that investment encourages growth. If both variables are entered into a regression at the same time, it might be that the institutional variable does not show up as significant, despite the fact it does have an indirect impact on growth. Sala-i-Martin (1997) has designed a test for robustness that allows for this problem with Levine-Renelt that we will discuss later.

In other ways, however, the Levine-Renelt test is not restrictive enough. Returning to the potential problem that cross country regression analysis assumes that coefficients and causal relationships are the same across countries and periods, the Levine-Renelt test is only performed on a one-period global sample. On a related note, the test does not allow for interactions between variables (for example, it might be that investment is only causally related to growth in the presence of a strong institutional structure). To some extent, this problem can be overcome within the regression framework—one can use interaction terms, or one variable multiplied by another, to investigate the differential impact of (as it might be) investment in poor and good institutional environments. Later, we will look at splitting country samples as another method to look for changes in the strength and direction of causal relationships. But it should be noted that growth models rarely include the presence of interaction effects even at this simple level, and it is such models that we are evaluating in this paper.

A further problem with the Levine-Renelt test is that it measures the robustness of correlation, but says little about causation between variables. Returning to the relationship between investment and growth, the evidence presented so far suggests that it is a strong (perhaps the strongest) correlate with growth that we have. Although it clearly misses the great majority of the growth story, this might suggest some support for a Harrod–Domar type model. Correlation does not, however, prove causation. In fact, a different statistical test performed by King and Levine (1994) suggests that, if anything, the relationship appears to operate the other way—growth encourages companies and people to invest more, rather than investment speeding growth. [13] Again, it should be noted that the causation issue can be examined using regression analysis as well, and there are techniques to (at least partially) overcome the effects of bi-causal relationships of the type investment-causes-growth-causes-investment. [14] Nonetheless, it suggests the need for yet more caution in interpreting regression results in the absence of such tests.

Turning to a broader analysis of the literature on the econometric study of growth, different formulations, different proxies and different combinations of a wide range of factors have been subjected to many millions of tests in many thousands of papers produced using cross country growth regressions. A list of variables that have been put into modern cross country growth regressions would include well over 100 overlapping economic, policy, structural, sociological, geographical and historical factors seen to influence economic growth directly or indirectly. Yet a brief survey of surveys appears to suggest that the results of all this computation have been disappointing—even when holding to standards lower than those of Levine and Renelt.

Education is a staple of almost all more recent discussions of economic growth. It is, however, found to be a particularly fragile causal variable by Pritchett (1996b), who uses a range of regression tests to find a relationship that is sometimes significantly negative. Of course, a range of arguments that we have seen above concerning the reasons why the invest-

ment to growth relationship might not be positive apply as much to significant negative results—our only point here is to argue that the relationship is certainly not robustly *positive*. [15] Research and development expenditure (another staple of some new growth theories) does little better at explaining growth—it seems to have no relationship with economic growth in poor countries and suffer from reverse causation in OECD economies (Birdsall & Rhee, 1993).

Looking at the relationship between trade and growth, both Edwards' (1993) survey and Maurer's (1994) survey found no strong evidence of a trade to growth linkage. A more recent survey by Rodriguez and Rodrik (1999) finds considerable weaknesses in the econometric literature on trade and growth since then, and concludes that "(w)e find little evidence that open trade policies in the sense of lower tariff and non-tariff barriers to trade are significantly associated with economic growth" (see also Walde & Wood, 1999). [16]

Looking at fiscal and monetary variables, Tanzi and Zee (1996) conclude that evidence on the relationships between tax policy, public expenditure, and debt neutrality and growth is inconclusive, [17] while Bruno and Easterly (1995) were unable to find a long-term relationship between inflation and growth. Similarly, institutional variables have weak predictive powers when it comes to explaining growth. Przeworski and Limongi (1993) and Brunetti (1996) both find that political regime type (democratic versus authoritarian) has very little explanatory power in growth regressions, and Brunetti also provides evidence on the weakness of government instability, political violence and policy volatility as determinants.

A range of other studies that have not relied on advanced statistical techniques have also pointed up simple problems with some recent theories that emphasize the importance of specific policy or institutional variables. Over the long term, US growth rates have been remarkably stable (we can predict US GDP in 1987 to within 5% simply by forecasting using the growth data from 1929 and the average growth rate from 1880–1929). Further, between 1870 and 1989, two-thirds of the present high-income countries have GDP growth rates within 0.2% of the US rate (Pritchett, 1996a). [18] It appears, then, that policy changes over time and policy differences across countries within the OECD and in wider samples of countries have had almost no effect on very long-term growth rates. A linked problem faced by policy-based explanations of growth is the predictability of income based on past income. If reasonably unconstrained policy choice explains the rate of growth, and some countries have followed better policies than others, then the rank ordering of country incomes should change dramatically over time, and past income should be little related to present income over the long term. In fact, neither of these statements is true, and past income is a very strong predictor of present income even over the very long term (Kenny, 1999).

Over the shorter term, Jones (1995) studies OECD countries in the postwar period. He documents that trade openness, durables investment, average years of education and literacy rates have all risen. According to theories of growth that suggest these are significant determinants of economic performance, the OECD should have seen increasing growth rates. But, what change there has been in these rates has been downward. [19]

At even shorter, 15-year periods, Easterly, Kramer, Pritchett, and Summers (1993) note that growth rates in a global sample of countries are in fact highly volatile while many of the causal factors we usually look at, such as policies and institutional structures tend to change rather slowly over this time frame. Figure 4 shows the relationship of growth rates over two of the decade periods commonly used in regression analysis—as can be seen, the relationship is fairly weak. Compare this to the correlation of an education variable (the log of one plus average years of schooling in the population) across time in Figure 5. [20] In other

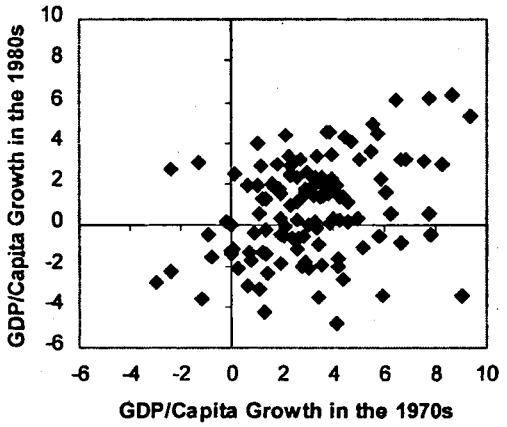

Figure 4. *The relationship between growth over decades.*

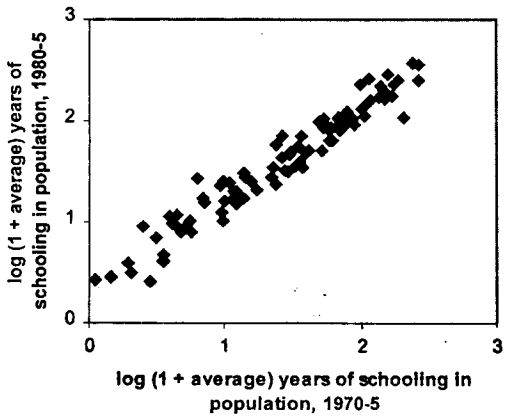

Figure 5. *The relationship between schooling over decades.*

words, policies vary too slowly to explain the volatility of shorter-term growth rates, and too fast to explain the stability of longer-term growth rates.

In addition, Stiglitz (1999) notes the problem posed for models which tie growth to policies (or, indeed, institutions) by the Chinese and Russian growth experience over the last decade. Since 1989, China's GDP nearly doubled, while Russia's almost halved. This suggests that the more rapid reform path taken in Russia—shock therapy—was not a success. Growth models based on the Washington Consensus or North's market institutionalism would suggest that Russia's growth rate should have increased as it moved toward more market-friendly policies and institutions. On the other hand, drawing any strong conclusion against shock therapy comes up against the problem that Poland, the Czech Republic and Hungary all introduced stronger reforms than Russia and have since faired much better. Another problem faced by theories based on a primacy of national policy is the weak response of African countries to adjustment. Several African countries that have conscientiously implemented sound macro policies have still only averaged a growth of 0.5% per year over the last few years (Buckley, 1999).

The huge regional inequality of income within countries that face the same macro policy framework is a final reason to suggest that there are factors at work other than those macro conditions and institutions usually (more or less formally) modeled. The rural poverty rate in Guizhou province, China, is 7-10 times higher than that in Guangdong province, for example. [21]

As of yet, then, we do not appear know whether policy or institutional changes have any major effect on long-term income levels, and if they do, whether they cause a one-off increase or decrease in output, or whether they lead to a permanent change in long-term growth rates. [22] Given that many theories of economic growth provide such firm conclusions on the universal relationship between variables and growth, it may come as a surprise that the econometric evidence is so disappointing. This is especially so given that nearly all econometric work on growth has been "playing tennis with the net down" (Blaug, 1992): attempting simply verification rather than falsification—proving that the evidence does not contradict the theory, rather than that the evidence proves the theory. [23]

Why is this? First, our relationships may appear weak because so much of our data is so weak (Kamarck, 1983; Leontief, 1971). Deaton and Miller (1995) find that, for a sample of African countries, GDP figures from two different sources over time have a correlation coefficient of less than 0.5 (the sources are Summers and Heston and the IMF). Killick (1992) finds that, between 1986–89, the IMF estimates that African exports grew 0.3%, while the World Bank estimated the figure at 3.2%. Second, some of our results will be weak because many of our regression results are based on faulty econometric techniques—although it is not always clear what the right techniques are (Evans, 1996; Caselli, Esquivel, & Lefort, 1996; Levine & Renelt, 1991; Mayer, 1992; Blaug, 1992; Pritchett, 1998). [24] Third, there are things that some are fairly confident are linked to the growth process, but that we have struggled to find a measure or proxy for (knowledge and innovation, for example). Linked to this, weaknesses might persist because we are unclear with our terms, or our terms are inherently unclear. Kamarck (1983) points out that African mines that feed miners a proper diet for several weeks after recruitment are increasing consumption expenditures—but in a way that definitely increases human capital. Quite possibly, indeed, it raises capital in a way that has a higher return than "investment" expenditures on added equipment. In a regression-related example, Pritchett (1996c) notes that the six trade variables frequently used in the growth literature are largely uncorrelated across countries. [25]

Finally, and fundamentally, we have seen that the relationships might appear weak because when we run large-scale cross country regressions, we are assuming a global common system of causal relationships between variables and output that might not exist at any level that can be caught using a cross country regression. Indeed, Stewart (1991) and Grier and Tullock (1989) both find strong support for the contention that global samples should be split by region, because relationships between variables and output change sign and strength of correlation across groups. For example, Stewart found that when running the same regression in Latin America and Africa, the population growth variable appeared to be a positive and significant cause of economic growth in Latin America while being a negative and significant cause of growth in Africa. Campos and Nugent (1999) find that the institutional variables most closely associated with growth vary between an East Asian and a Latin American sample of countries. Limiting studies to one region does not appear to solve the problem, however. Four studies at the start of this decade performed regressions on a purely Latin America sample (De Gregorio, 1992; Stewart, 1991; Cardoso & Fishlow, 1989; Grier & Tullock, 1989). Four variables were used by more than one study, and of these, only one (export growth) was found significant in all of the studies in which it was used (in this case, just two studies). Depending on time frame or what else we take account of in our studies, policies or economic characteristics of a country that look terribly important and positive in the growth process in one study frequently look largely irrelevant or negative in another.

Perhaps econometric samples should not only be split by region, subregion or even to the individual country level, but perhaps they should also be split by time. For example, it might be that the oil crisis had a significant structural effect on the causal processes of economic growth. In the short term, it does appear that the strength of relationships and growth is very dependent on the period one studies. [26] Longer term evidence for the importance of period is provided by studies of 19th and early 20th century growth, where Harley and Crafts (1998), Morris and Adelman (1988) and Taylor (1996) find that influences on growth posited by modern theories have very little explanatory power in explaining 19th century growth (further, Adelman, 1999, points out that there is not one model that can explain all 19th century growth, either). This might not be very surprising. The economy of present-day Pakistan looks nothing like the 1820s economy of the United Kingdom—despite the fact that their GDP per capita is broadly similar ($1,642 for Pakistan in 1992 as opposed to $1,756 for the UK in 1820 Maddison, 1997). Two obvious reasons are that much of Pakistan's capital stock involves goods not yet invented in 1820 and that the relative world prices of goods have changed dramatically over that time. [27]

Samples could perhaps be split by sector as well. Bernard and Jones (1996) find evidence that convergence results in OECD countries are driven by services, while manufacturing shows no sign of convergence. This suggests the two sectors might work very differently. Again, however, splitting by sector alone again might not be enough. Mundlack, Larson, and Butzer (1997) report strong evidence against a global production function for agriculture, for example. But if the only way to get a "robust" result on the causes of economic growth is to split our studies by country, period and sector, this suggests that the economics of an advanced industrial country such as the United States in 1960 can tell us little about the economics of an agricultural country such as Kenya's in 1960—and even less about the economics of Kenya today. Not only does the evidence suggest little support for an assumption of homogeneity, then, but to the contrary, strong evidence in favor of heterogeneous processes at work.

All of this is not to say we know nothing about the growth process. First, we have some evidence that institutions and/or structural factors matter in explaining the "mean" around which growth rates perform their violent random walk. In one of the most ambitious recent attempts to find some "robust" variables, Sala-i-Martin, in his (1997) four-million regression test paper, finds 21 variables that he considers robust to a less rigorous version of Levine and Renelt (1992) analysis. The test is based on the idea that if correlations are nearly always (95% of the time) of the same sign even allowing for different "conditioning sets," it suggests that variables have a strong positive (or negative) relationship with growth even if the correlations are not always statistically significantly related to growth. [28] It should be noted that Sala-i-Martin's variables are only shown to be weakly robust in a one period regression using a largely constant set of countries. Despite this weakness in the test, it might be interesting to look at what survives.

The variables that survive the process include those linked with religious affiliation, region (a country being located in Africa, for example), primary goods orientation (exporting agricultural and mineral products), latitude (being near the equator), political and civil rights, the rule of law, war, revolutions and coups, investment, foreign exchange variables, a country's "degree of capitalism" and numbers of years the country can be classified an open economy. Fully half of these variables can be considered "structural," and only two can be considered "true" policy variables. These policy variables are both general measures that capture a multitude of sins—degree of capitalism and number of years an open economy—and are open to dispute as fair measures of policy orientation. [29]

This might suggest that structural variables that governments have trouble changing, such as religious beliefs, or geographic location, or the quality of land, play an important role in the long-term growth process, something also supported by a number of recent studies including Sachs (1997a,b), Easterly and Levine (1997a), Kenny (1999) and Hall and Jones (1996). Again, the results also fit with evidence presented earlier suggesting that a country's growth performance tends to look like a random walk around a mean— the mean is, perhaps, set by structural factors. [30] This is not to argue that a structural constraint is and will remain always a hurdle to growth whatever the period, sector, level of development or other constraints present. It is merely to note that factors beyond the power of man (or government) to change could be important elements in determining growth rates over the long term, and that, given the importance of structural variables, it is quite possible that different policy mixes might be optimal in the presence of different structural constraints.

There are a number of other factors that appear fairly robustly associated with growth. It is a near-universal phenomenon that the proportion of labor in agriculture declines as countries become richer and that manufacturing and services contribute more to GDP (Syrquin & Chenery, 1989). We know that consumption of food as a percentage of total expenditure drops rapidly as people become wealthier. [31] We have weaker evidence of changes in attitudes (Inglehart, 1995). What we do not appear to know, and what global sample cross country econometric studies have not been able to tell us, are the causal relationships at work and the processes that link factors to outcomes.

4. DISCUSSION

It is a familiar (and justified) refrain that theories or models of economic growth must simplify the world they attempt to depict. Indeed, most if not all growth models are only designed to be partial rather than complete models of the operation of economic change. It would seem to be a condition of good models or theories, however, that they do in some way actually represent, albeit in a simplified form, that part of the world they are concerned with, capturing enough of the important causal real world relationships to have explanatory and predictive power.

While there are problems and inadequacies in the statistical techniques frequently used to assess these theories, the universal failure to produce robust, causally secure relations predicted by models might suggest a broader problem than statistical methodological weaknesses. [32] The evidence appears to suggest that country growth experiences have been extremely heterogeneous, and heterogeneous in a way that is difficult to explain using any one model of economic growth. In this section we return to the connections between epistemology and ontology and discuss two related problems with the way in which economic growth has been theorised—their ahistoricism and their accounts of causality.

First, despite the fact that economic growth is a process which takes place over often extended periods of time, most models or theories of economic growth are in an important sense ahistorical. Apart from a few exceptions they tend to assume that the process of economic growth is the same the world over and through time (because the components and process of all economies are the same).

Why might this ahistoricism be a problem? Easterly (1997a) points out that there are probably virtuous and vicious cycles at work in development, connected with "threshold effects" (an idea that dates back at least to Rostow, and was revisited in Chenery, Elkington, & Sims, 1970). For example, if investment has a higher return in the presence of large stocks of capital because of various externalities to capital accumulation (a central element of Romer's thesis, looked at earlier), then if a country has a certain critical mass of human

and physical capital to start with, growth will be a virtuous circle of capital accumulation attracting yet more capital. But a terms of trade shock, for example, that rendered part of a country's capital stock useless might shift some countries near the critical mass level of capital below the critical line, so shifting it from strong growth to decline. On the other hand, the same shock would have little long-term impact on countries distant from the threshold.

Ravallion and Jalan (1999) note another possible cause of a vicious cycle: "mixed" goods involving both public and private benefits (such as health care) will be less well supplied in poor areas. Agriculture is also full of examples: returns to insecticide spraying are highly dependent on water availability, early planting, weeding and fertilizer. African field trials suggest that returns to insecticide spraying are eight times higher in the presence of these other factors than in their absence (Carr, 1993). In order to escape a vicious cycle of low returns to agriculture, then, farmers need to be able to implement a range of new practices all at once.

The presence of virtuous and vicious cycles itself raises questions of long-term path dependency (and so historically-situated interpretations of the growth experience). A small difference in the initial capabilities or conditioning sets of a country could send it off on very different growth trajectories. But this also raises the question of interaction effects across international borders. We have seen that the global and neighborhood environment sometimes appears to have a significant impact on growth outcomes—and it might even be that these effects dominate long-term development outcomes. While the dependency view of history tends to be fairly linear and universal, in common with the standard views of economic growth, it does suggest the importance of looking at the dynamics of growth at levels other than the national. Indeed, it is possible that the global economy is a complex system—one that is impossible to dissect into component parts because the system itself arises from the interactions among those parts, involving any number of feedback loops that produce counterintuitive results. This would bring into question the validity of cross country growth regressions where the country is the unit of observation. [33] If this is true, it would also make it impossible to produce a satisfactory predictive model of a particular sector of a particular type of economy.

Regarding causality, most growth models could be said to have perhaps an over-simple causal account at their heart. John Stuart Mill argued that the "cause" of any particular event is the set of conditions or factors which, taken together, are a sufficient condition for the event to occur (Mill, 1975, Book 3). That is, events rarely, if ever, have a single cause, but are rather the result of a conjuncture of several factors or conditions. Mackie (1980) has suggested that even this is not a adequate account of the complex causal nature of events, and has instead suggested that we view causation in 'INUS' terms. For Mackie a cause is any factor or condition that is itself an insufficient, but necessary, part of a conjuncture of factors or conditions that are unnecessary but sufficient for the event to occur. To put it in terms of economic growth, it may be that increasing rates of investment is an INUS factor. It may be an insufficient but necessary part of a set of conditions which cause growth to occur, but that growth is not necessarily always caused by this conjuncture of conditions.

Given the complex causal nature of the world, Mackie argues that scientific laws do not state "what always does happen," but rather "what would, failing interference, happen" (Mackie, 1980, p. 76). Given that, as far as we know, the social world is more causally complex than the natural world, and hence there are likely to be many more things which could "interfere" with the supposed causal connection, and given the evident impossibility of isolating causal relations in the social world in a "laboratory," it might be a mistake to think that there are any small number of INUS causes of economic growth which will work unaltered across countries over time. As Francois Perroux has argued,

> when a first class statistician establishes a parrallel between the growth of GNP and that of major aggregates—investment, consumption, savings, etc.—there is good reason to doubt whether what he is doing amounts to more than taking the first step towards a causal analysis (Perroux, 1983, p. 27).

More than 40 years ago, Gunnar Myrdal said much the same thing. He argued that economists concerned with economic growth need to accept not just that it may have a great number of causes, but also that these do not work in any "linear" manner. He suggested that problems like economic growth should be examined using the concept of "circular causation" where

a change in one factor would affect a number of other factors, and these changes would in turn feedback on the first factor (Myrdal, 1957, p. 16). The essence of a problem such as economic growth is that "it concerns a complex of interlocking, circular, and cumulative changes" (p. 14). For Myrdal this had two implications. First, it was "useless to look for one predominant factor, a 'basic factor'—as everything is cause to everything else in an interlocking circular manner" (p. 19). Second, viewing economic growth in these terms meant abandoning the search for neat econometric models: "the relevant variables, and the relevant relations between them are too many to permit that sort of heroic simplification" (p. 101).

The causal issues involved in economic growth are further complicated by the problems of accounting for the causal significance of peoples' beliefs about themselves, about others, and about the future. In contrast to atoms in chemical reactions, economic agents are conscious that they are part of an event, are conscious of others, and of past events, and this can effect their behavior. What people do in the social world is in part the result of what they think about other agents, what they think other agents think about them, and so on, and what they believe about the future. Again as Francois Perroux has argued, economics has yet to get to grips with the idea that individual economic agents are active, thinking persons, not simply through-puts in the working out of timeless and spaceless economic laws and relations (1983, pp. 69–70). But this injects an important element of indeterminacy into processes of economic change, which is not generally captured by the current way of thinking about growth. Sadly, there is little reason to think we will ever be able to grasp fully beforehand this kind of process. Over the long term especially, it suggests that the complexity of predicting the effects of policy or institutional change on economic relations or output.

Finally, as the ongoing debates about the Industrial Revolution in England show, even historically-centered, nonuniversal, complex causal models of growth cannot be conclusively proven—the historical record does not straightforwardly provide a single plausible account of any event.

Overall, while the work of the natural scientist is underpinned by an assumption that there really are a few general principles which structure the natural world, and while economic theory would suggest the same is true of the economic world, there appears to be little convincing empirical evidence that this is actually the case as regards growth. This might mean that current models only capture a supposed theoretical relationship that we have little reason to think will actually be manifested in the real economic world.

The above argument might suggest that mathematical modeling techniques have invaded territory to which they are ill-suited, imposing unrealistic criteria of rigor and precision. [34] We do not want to embroil ourselves too far in the debate over formal modeling in economics, between those who see modeling as the "mental gymnastics of a particularly depraved mind" and those who see non-mathematical treatments as "fat, sloppy and vague" (Samuelson & Klein, in order, quoted in Roy (1989, p. 140)). Many early development economists did not use mathematical models (Lewis'book is almost completely absent of equations—although he did have a role in starting the trend toward more mathematical treatments), but while some early and most later development economists *did* use models, there is apparently little difference between them in terms of their predictive and explanatory ability.

In judging the utility of complex models, we would, however, argue with Krugman when he claims that self-conscious formalization leads to better results. As he points out formal modeling cannot easily handle complex ideas—only recently has it been able to encompass the idea of increasing returns, and it still struggles with imperfect competition. Krugman's argument that the "essential logic" of high development stories can emerge in a simplified setting using models (Krugman, 1997) is perhaps close to a truism, and suggests that we will lose the important nuances which appear vital in accounting for growth outcomes. To argue that we are lost in the fog without models might be to wish the all too real fog away. Because the formal models tend to be even more parsimonious than descriptive theories about development, it is perhaps not unsurprising that they have been, if anything, even less helpful in illuminating what appears to be a highly complex and varied phenomenon. [35]

Perhaps one could develop a model that allowed for the heterogeneity, and took into account all of the interactions between variables, but that model could not be tested because (as we have seen in regard to trade) our

theoretical concepts are loose, because there are not (and could not be) the undisputed econometric tools to declare such a model uniquely and causally congruent with the data, and because there is certainly not the quantity of accurate data required to test it. Further, our experience with short-term predictive models of growth might give us pause in such an exercise. It appears that models that use thousands of variables to predict economic conditions a year or two out are no better than those that use only one or two variables (Sherden, 1998).

Heterogeneous growth experiences are only a surprise if we think that "underneath," as it were, the components and process of all economies are the same, and that the complex interaction of external conditions, noneconomic factors and peoples' beliefs have no real causal importance in explaining growth experiences. We have argued that there are good empirical and theoretical reasons for thinking this is not the case. Instead,

> we are faced at every turn with the problems of organic unity, of discreteness, of discontinuity—the whole is not equal to the sum of the parts, comparisons of quantity fail us, small changes produce large effects, and the assumptions of a uniform and homogeneous continuum are not satisfied (Keynes, 1993).

5. CONCLUSION

There are, we think, a number of conclusions and implications which follow from the analysis we have presented here. First, a review of the available evidence suggests that the current state of understanding about the causes of economic growth is fairly poor. Clearly there have been development "successes" just as there have been development "failures." What we are arguing is that we are in a weak position to explain why some countries have experienced economic growth and others not. This should, we think, induce us all to be a little more cautious in the certainty with which we hold to present models and modes of thinking.

In part because of this (but also because the future is sure to be different), we do not think that there is any very firm empirical basis for the confidence with which (any) development practitioners advocate particular policy prescriptions. The practice of development has relied upon theories which all purported to explain why developing countries had not experienced economic growth, and purported to be able to offer cogent advice on what should be done to overcome this. Yet the available evidence suggests that none of these theories has so far been able to do this very well (even when playing the far easier game of explaining past growth rather than improving future growth).

Even if some might argue our selective review has been overly pessimistic in its evaluation of the output from cross country regressions, the concerns we have raised over the leap between correlation and causation, the quality of data, the looseness of concepts and the sheer number of variables that have been put into regressions should at least suggest the fear that policy recommendations are sometimes nothing more than a mechanical output of the particular choices made by the model-builder.

The possible dangers inherent in the use of regression analysis as a source of policy advice can be illustrated by looking at the use of sometime significant coefficient estimates. For example, different studies suggest that the following will increase per capita growth by 1%: increasing the average years of schooling in the labor force by 1.2 years, increasing the ratio to GDP of public investment in transport and communication by 1.7%, a fall in inflation of 26%, a reduction in the ratio of the government budget deficit to GDP of 4.3% (Easterly, 1997a). But, first, we know that these results are not robust—that the size and significance of the coefficients change dramatically across samples. Second, we know one of the reasons for this lack of robustness is that these variables are linked with many others. It might be that changing policies to improve these variables is important to growth outcomes, it might be that they happen to be usually correlated with the factors that really matter, and intentionally trying to change them without altering the truly significant correlate will have no effect on growth. There might also be interactions between the variables. For example, if the government uses budget deficits to finance transport and communications, what happens to growth? It might also be that these policies and outcomes are significantly linked to the growth process in one country at one time, but not significantly linked in another country at another time, or that different initial conditions completely reverse the effects of policy change at different times in different places. The inflation variable shows up the importance of accounting for nonlinear relationships between

variables. If reducing inflation by 26% really increased growth by 1%, this suggests that Brazil's GNP per capita growth rate would have gone from 5% in 1994 when the country had an inflation rate of 2,948% to a per capita growth rate of somewhere in the region of 118% in 1996 when the inflation rate had dropped to 16%.

On the other hand, many of the variables found unrobust or insignificant in cross country regressions might in fact have a large effect on certain countries at certain times while being insignificantly or negatively related in other countries or at other times. The danger of relying on such studies for policy advice is not just one of commission, then.

We would argue that this should encourage development practitioners to exercise caution in using coefficients for anything more than the most tentative of illustrations. At the same time, this study might suggest the need for caution in assuming that the "lessons" from the development experience of one sector or one country at one time can be applied substantially unaltered in different sectors or different countries at later times. To quote an early development economist:

> all of this is not an argument against economic development. But it shows the necessity of applying Western experiences and 'rational'methods only with great caution and with a real effort to understand the culture, value system and social structure of each population group that is to be exposed to a development policy (Lauterbach, 1957).

We know that in practice this is an effort far more widespread than it is in theory (Cohen, 1995), but to the extent that theory drives practice, it is an effort that should be made more open and more central to theoretical treatments.

It should be noted that some recent treatments of the development process are re-emphasizing its complex and interlinked nature, perhaps moving the field back to the more holistic treatments of the 1950s and before (Stiglitz, 1998). While it can well be argued that saying everything is important has little practical use, it might be a more accurate characterization of the development process than focusing on one or two keys to development success. As we do not seem to know exactly what we are aiming at, it might be better to use a shotgun than a rifle, then.

On the theoretical side, perhaps, more energy should be directed toward understanding the complex and varied inner workings of actual economies rather than trying to assimilate them into abstract universal models. Over 100 years ago, Thorstein Veblen said substantially the same thing. He argued then that economists needed to abandon their animistic bent, and instead turn to a study of the complex causal processes of economic development (Veblen, 1948). Even historical accounts of economic growth in particular places will not necessarily lead to conclusive causal narratives, but it might at least allow us to escape from some of the disabling commitments which characterize crosscountry statistical investigations into growth. It would perhaps allow for the development of more appropriate interventions, tailored more closely to the particular circumstances of particular countries.

NOTES

1. The authors would like to thank two anonymous reviewers for their helpful comments.

2. We mean no disrespect to Jeffrey Sachs, a widely known, widely respected and highly prolific economist, who has produced some of the most cited recent work on economic growth (Asian Development Bank, 1997; Sachs & Warner, 1995, 1996) and has played a vital theoretical and practical role in our dealings with economies in transition (Sachs, 1993). Regarding the two articles, Sachs argues that, while point estimates and some important implications changed from one paper to the next with the addition of geographic variables, the important benefits of good policy remain significant—the articles are different, but not contradictory, then (correspondence with authors, June 23, 1998).

3. In defense of World Bank economists, they no longer actually believe in the model as a helpful representation of the long-term growth process—if they ever did. On the other hand, the model still crops up in discussions of country growth prospects in World Bank documents and elsewhere (noted in Easterly, 1998; Deverajan, Easterly, & Pack, 1999).

4. A cynic might note that this list moves from the relatively simple to overcome toward the impossible to change (even more so if we take the story in to the later 1990s and add geographic factors).

5. We do not want to enter into the detailed debates about methodology in economics. Most economists tend not concern themselves with such issues, but we think that most would accept the rough and ready view that economic theory is to be "judged" against the empirical evidence.

6. Studies with similar findings include Pons (1999) and Greer (1999). A recent test of the World Bank's projections for developing country GDP growth one year ahead do suggest that they are better than the naïve forecast—but only marginally (Verbeek, 1999).

7. Another example of an early development economist who seems to cover most if not all of our present concerns is Lauterbach (1957). He specifically mentions: savings, population growth, barriers to trade, lack of credit organizations, political traditions, ethnic and religious division, civil disorder, inflation, income distribution, communications systems, crime, public health, education, technology diffusion, property rights, exchange controls, types of government, corruption and beliefs. Moreover, Lauterbach and Lewis both stood on the shoulders of generations of earlier economists (and others) who had exploited similar ideas. Sachs (1997a,b) quotes Smith in support of both his contention that a small government is the sufficient condition for growth and to back the idea that geographical conditions can be a serious impediment to development. Smith (1976) also argues that inequitable landholding is bad for economic development. Heilbroner had not forgotten the question of the development capacity of tropical countries in 1963, indeed he argued that: "Not many years ago the prima-facie 'evidence' made the climate theory of underdevelopment virtually the prevalent explanation of economic backwardness." The idea that savings and investment are important to growth goes back even before Smith, to the Mercantilists (Mason, 1998). On human and social capital, in *Anna Karenina* Tolstoy writes about what was clearly an active debate in 19th Century Russia over the reasons for the backward state of the country—covering topics such as education, insufficient market reforms to encourage peasant entrepreneurship in the countryside, authoritarian versus participatory methods of agricultural development.

8. The data are from Summers and Heston (1991). These data and regressions presented in the following notes are based on all available data from a 160-country sample (that of Easterly & Levine, 1997a).

9. Regressing decade average GDP growth on a constant and decade average investment, $N = 425$, $C = 3.255$, $\beta = 0.065$, the t-statistics on investment is 4.83, Adjusted R^2 is 0.05.

10. With average investment as the dependent variable, $N = 439$, $C = -27.9$, $\beta = 5.9$, the t-statistics on log initial income is 17.4, Adjusted R^2 is 0.41.

11. In a regression of growth against a constant, log initial income and investment, $N = 423$, $C = 11.2$, $\beta = -1.13$ (t-stat. -8.3), $\chi = 0.15$ (t-stat: 9.35), adjusted R^2 is 0.19.

12. Fuentes' (1995) selective review of the growth literature also argues (in part based on the Levine and Renelt results) that available estimates of the technology and convergence parameters appear to be quite sensitive to sample selection, econometric specification or even the list of regressors included in the regression.

13. The arguments that rage over growth accounting exercises, which look at the relationship between stocks of human and physical capital and GDP levels, are a further sign of the disputed link between investment and growth—and the role of human capital and technology. Depending on the exercise one chooses to believe, the East Asian growth miracle can be explained almost entirely in terms of high rates of physical and human capital accumulation (or investment in goods and education), or largely on improvements in the productivity of human and physical capital due to technology adaptation and advance (see, for example, Young, 1995 & Crafts, 1998—on the side of capital—*versus* Klenow & Rodriguez Clare, 1997—on the side of total factor productivity). Even using similar methodologies (weights and procedures) the measured contribution of factors varies greatly by country and period (Crafts, 1999). Felipe (1999) concludes his survey of TFP growth and capital accumulation in East Asia by arguing that "this work has become a war of figures. From the crudest calculations to the most detailed studies .... In many cases these are straight exercises in data mining embedded in fancy empirical methods... It seems that re-working the data one can show almost anything."

14. Techniques to test causation include the Granger Causality Test, two simple (and somewhat problematic) measures to overcome bi-causality include using initial values of variables rather than period averages and the (slightly more advanced) two-stage least-squares technique.

15. A non-econometric exercise can emphasize the point. Worldwide, school enrollment and life expectancy have shot up over the last 30 years—on average, life expectancy has risen by four months each year since 1970, while adult literacy has risen from 46% to 70% over that same period. These increases have been felt on every continent and in nearly every region (World Bank,

1999). Yet what movement we have seen in worldwide growth rates over that period has been downward.

16. The dominant theories also suggest that periods of open trade policies among countries should see convergence in incomes between them—so that the dispersal of income levels should decline over these periods. The Rodriguez and Rodrik (1999) study suggests, instead, that periods of high tariffs (the depression and the interwar period) saw convergence in country incomes, while times of freer trade (the period up to 1878 and the postwar period) have seen divergence of income. Further, the relatively open East Asian economies have seen divergence since 1960 while the relatively closed Latin American economies have seen convergence.

17. Rather worryingly, they nonetheless conclude that, despite the lack of empirical support, trusted models should still be used.

18. Clark noted this same phenomenon as early as 1944. He pointed out that even WWI had only a "comparatively slight and temporary effect" on long-term trend growth rates. As we have seen, the same is true of WWII, suggesting that, even when countries are doing their best to have an impact on output, they have only a fairly limited long-term effect (Clark, 1944, p. 4).

19. Pack (1994) also concludes that recent growth models do poorly when tested against events such as the productivity slowdown in the OECD countries or the Asian growth acceleration.

20. Data for graphs come from Easterly and Levine (1997a).

21. The reforms introduced in China since the late 1970s have apparently slowed the rate of convergence across regions of China, but cannot be blamed for the extent of the initial disparity of incomes (Ravallion & Jalan, 1999).

22. More broadly, the two most basic, and linked, questions about the nature of growth remain disputed: is there convergence and are there decreasing returns to capital? The answers to these questions depend on who you ask and how you phrase the question (Kocherlakota & Yi, 1995; Quah, 1993; Evans, 1996; Barro & Sala-i-Martin, 1992; Parente & Prescott, 1993). The evidence apparently does not support a firm judgment between the conclusions of neoclassical variants, new growth variants or structuralist variants (see Young, 1995 *versus* Romer, 1993 on object *versus* idea gaps).

23. We would accept that there are huge problems with falsification in economics—one reason that it cannot be the type of science that some practitioners hope for it. It is near impossible to impose the necessary controls to solve identification problems or to ensure the use of the "right" model (McCloskey, 1983).

24. For example, Caselli *et al.* (1996) introduced a generalized method of moments estimator to overcome inconsistencies in previous crosscountry-regression work connected with correlated individual effects and endogenous explanatory variables. Pritchett (1998) argues that their technique is itself flawed, however, due to the volatility over time of growth in a country.

25. The variables are frequency of nontariff barriers, average tariffs, structure-adjusted trade intensity, an openness index, a trade distortion index, and price distortions.

26. This can be illustrated by studying Easterly and Levine (1997b) Table IV, Column 4 regression (used because the data were near to hand). This is a 1960-90 regression of decade growth in GDP per capita against an Africa dummy, a Latin America dummy, log initial income, log initial income squared, ethnolinguistic fractionalization, log of years of schooling per capita, assassinations per capita, financial depth, the black market premium and government surplus. Running the regression one decade at a time (without the decade dummies) the variables that are significantly related to growth in each period are: *1960s* the Latin America dummy, the income terms, the black market rate; *1970s*, the Latin America dummy, the income terms, ethnolinguistic fractionalization, schooling, the black market rate, the government surplus; *1980s*, the Africa and Latin America dummies, the income terms, the black market rate, the government surplus. Across periods, then, the only robust results from this regression are the Latin America dummy, initial income and the black market rate (which might be proxying for terms of trade changes)—although some loss of significance should be expected from the attenuation of t-stats given smaller samples.

27. DeLong (1992) estimates that real incomes in the United States have risen eight-fold over 1895–1990. Incomes in terms of purchasing power of particular goods have shown very different changes, however. In terms of the number of mirrors the average American can buy, US citizens are 60 times richer today than in 1895. In terms of the number of silver teaspoons, the average American is actually poorer (by about 10%) than they were in 1895.

28. It will be remembered that Levine and Renelt's test looked at whether the variables remained *siginificantly* positively or negatively related in the presence of different conditioning sets.

29. One measures "capitalism" at period end, leading to questions of causality, the other is a composite measure that includes a variable from the mid-1980s, a variable calculated only for Africa, and a black market rate variable which is probably connected with terms of trade shocks (see Easterly *et al.*, 1993).

30. This also gives more credence to the explanations of the present distribution of global wealth argued by Landes (1998) and Diamond (1998)—arguments that, in Landes'case, involve factors going back to at least the Renaissance, and in Diamond's, back to approximately 10,000 BC.

31. Having said that, there appears to be a surprisingly weak relationship between economic growth and many desirable outcomes that we have traditionally assumed went along with higher incomes. For example, economic growth does not appear to have been behind the worldwide improvement in life expectancy, or improved political rights, or access to sanitation and clean water (Easterly, 1997b).

32. It is possible that we have missed the causally significant variables—but if it is true that we have been looking in all the wrong places for the development silver bullet, this too suggests we need to re-examine our theories.

33. Some economists are now trying to model such global systems, spurred on by Krugman's work in the field of the economics of geography (Krugman, 1997). Examples include Matsuyama (1996) and Baldwin, Martin, and Ottaviano (1998).

34. As Frank Hahn, the president of the Econometric Society in 1970, complained about econometric models in general: "there is something scandalous in the spectacle of so many people refining the analysis of economic states which they have no reason to suppose will ever, or have ever, come about" (Mayer, 1992, p. 2).

35. To take but one example, Romer's (1987) model predicts that labor growth will be bad for long-run growth because increased labor supply reduces firm's incentives to invest. This might sound plausible for manufacturing. In agriculture, however, there is plenty of micro evidence to suggest that increasing labor pressure is vital to spurring investment and advance through land-saving technologies (Tomich, Kilby, & Johnston, 1995, p. 385).

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